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PHYSICAL CHEMISTRY.—*A convenient form of autoclave.* GEORGE W. MOREY, Geophysical Laboratory.

The following form of autoclave has been designed after considerable experience with autoclaves and bombs, and is believed to be superior to commercial forms in ease of construction, in manipulation, and in certainty of closure.

It consists of three parts: a photograph of these separate parts is shown in figure 1, and the assembled autoclave, in cross-section, in figure 2.

The cover, *B*, of the bomb, *D*, is held in place by being pressed against a projecting rim, *A'*, on the outer shell, *A*, by the bolt, *EE*. Following is a detailed description of each part.

The outer shell, *A*, is made from a piece of 4-inch standard pipe. On the upper end a shoulder, *A'*, $\frac{3}{8}$ inch thick, is welded with an oxyacetylene blow-pipe. Near the lower end of the pipe two opposite holes, $1\frac{1}{4}$ inches in diameter, have been drilled; their centers are $6\frac{3}{8}$ inches from the top. These holes are shown in section in figure 2, and are more plainly seen in figure 1.

Inside the outer shell is placed the bomb, *D*. It is made from a piece of $3\frac{1}{2}$-inch standard pipe, one end of which is closed by welding on an iron bottom, as shown. The other end is closed by means of the cover, *B*, the outer rim of which is pressed against the rim, *A'*, on the outer shell.

Special attention should be called to the type of closure. The washer, a ring of 0.8 mm. gold wire, is placed in the space, *C*,

where it is completely surrounded by metal, the sides of the cover being a close fit. This type of closure is the same as that described in a previous paper[1] for closing bombs at high pressures and temperatures. As emphasized therein, it is essential that the washer be completely enclosed by metal, so that when pressure is applied it cannot flow away.

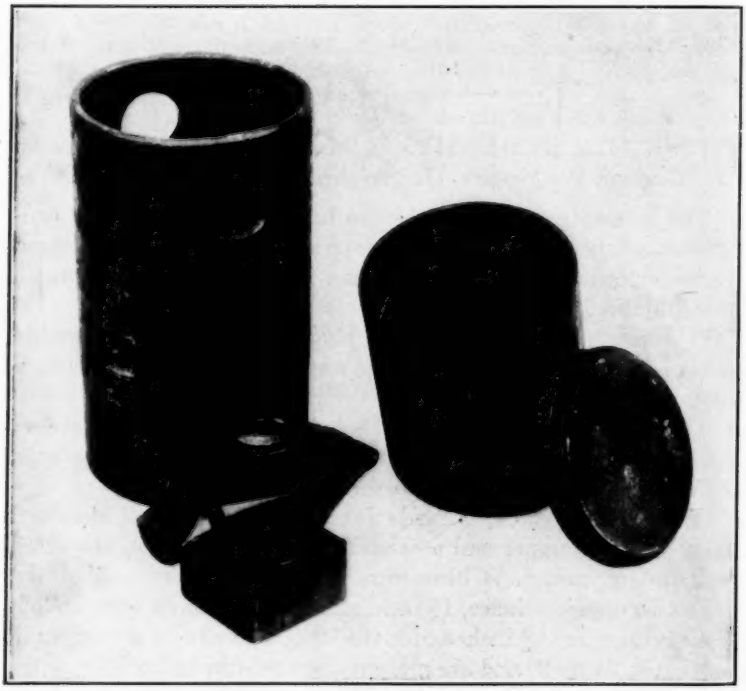

Fig. 1. Photograph of the parts of the autoclave

The cover is held in place by being pressed against the projecting rim of the outer shell by the bolt, *EE*, which is threaded

[1] MOREY. Journ. Am. Chem. Soc., **36**: 215–30. 1914; Zeits. anorg. Chem., **86**: 305–24. 1914.

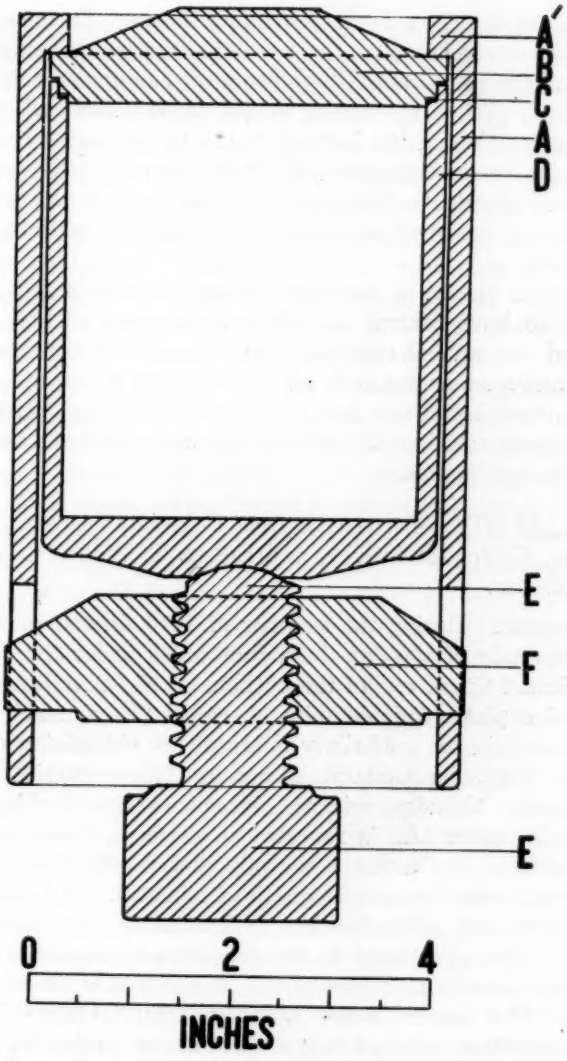

Fig. 2. Cross-section showing the details of the construction and assembling of the autoclave.

through the cross-bar, *F*, in the manner shown. The ends of the cross-bar are semicircular, the arc of the semicircle being downward and pressing against the bottom of the circular hole in the outer shell. When the bolt is turned, its rounded end, turning in the depression in the bottom of the bomb, forces the cover against the rim on the outer shell, thus compressing the gold wire in *C*, and effecting a tight closure. The ends of the cross-bar are cut away as shown, to enable it to be inserted after the bomb is in place.

The cover shown in the sketch is easily made, and it is convenient to have several for different purposes. Thus, one is provided with an exit tube and valve to permit the escape of the vapor, and another has both an exit tube and a pressure gage.

The above autoclave has been used for many experiments with aqueous solutions at temperatures up to 300° and has given complete satisfaction.

MINERALOGY.—*Leverrierite from Colorado.*[1] ESPER S. LARSEN, Geological Survey, and EDGAR T. WHERRY, National Museum.

Occurrence. During the summer of 1916, in the course of a geologic study of the San Juan Mountains under the direction of Whitman Cross, one of the authors (E. S. L.) collected specimens of a platy, foliated mineral, called "clay-gouge" by the prospectors, in the gold-silver mines of the old mining camp of Beidell, Saguache County, Colorado. The material was in considerable abundance in the dumps of the Buckhorn and Esperanza mines and is reported to occur in irregular bodies up to several feet across. A study in the office has shown it to belong to the leverrierite group of minerals. It is associated with gold and silver-bearing psilomelanite, pyrolusite, and quartz. The pyrolusite is in well-formed prismatic crystals with very perfect cleavages and is probably derived from manganite. The quartz is not abundant and is partly in well-formed crystals, up to an inch or more across, projecting into the

[1] Published with the permission of the Director of the United States Geological Survey and the Secretary of the Smithsonian Institution.

leverrierite, and partly of cherty character in coatings and reniform masses attached to the manganese oxides.

Physical properties. The mineral has many of the properties of ordinary "clay-gouge," but it differs in possessing a prominent micaceous cleavage, some of the cleavage pieces being several inches across and commonly bent. When dry it is rather brittle and has a hardness of about 1½. If large fragments are immersed in water they gradually break up, chiefly along the cleavage, into smaller pieces. After soaking for some time the mineral becomes smooth and plastic, and rather sticky. Under the microscope the fragments can be seen, however, to have retained their optical properties. On kneading the plastic mass the fragments are broken up, but still retain their properties. The plasticity can not be accounted for on the basis of the presence of any colloidal material, but seems rather to be due to the superposition of innumerable minute, soft, flexible plates, separated by films of water. The Colorado leverrierite has a vitreous luster and waxy appearance, and varies in color from nearly colorless to dark olive-buff (Ridgway 21‴) or russet-vinaceous (Ridgway 9‴).

Optica properties. A microscopic examination of a number of specimens showed them all to be essentially identical. The pale russet-vinaceous specimen selected for analysis is in bent plates up to an inch across, and appears to be homogeneous. The acute bisectrix is normal to the plates, the axial angle is sensibly 0, and the mineral is optically negative. The indices of refraction were measured by the immersion method. Fragments immersed in mixtures of clove oil and petroleum oil and quickly compared with the oil gave apparent values of $\alpha = 1.470 \pm 0.005$, β and $\gamma = 1.515 \pm 0.005$, but on standing in the oils the indices of refraction slowly increased. After standing for two days in mixtures of clove oil and cinnamon oil they reached a maximum constant value and matched the liquids in which they were imbedded with values of $\alpha = 1.558 \pm 0.003$, β and $\gamma = 1.602 \pm 0.003$. On removing the fragments from the liquid that had stood and placing them in a fresh liquid of the same index they still matched.

A similar change in the apparent indices of refraction, as measured by the immersion method, has been noticed in a number of other minerals. In some it is clearly due to the fact that the mineral has submicroscopic pores and that the apparent index of refraction is the result of the combined effects of the mineral and gas or water filling the pores; but if the pores are filled with a liquid having the same index of refraction as the mineral, the true index of the mineral can be measured.[2] For instance, the halloysite ("isotropic kaolinite")[3] from Wagon-Wheel Gap, Colorado, is clouded when first immersed, and the liquid can be seen to penetrate the grains, making them clear and increasing their index of refraction. The pores in the leverrierite are submicroscopic and the mineral appears clear, but the penetration of the liquid can be followed by the change in index of refraction in a zone from the border inwards.

Chemical properties. Qualitative examination having shown the material to be a hydrous aluminium silicate, it was analyzed quantitatively in the laboratory of the National Museum. The behavior of the water proved to be rather peculiar, so its relation to the optical properties of the mineral was studied first.

Behavior of the water. It is customary in mineral analysis nowadays to determine water of two kinds. That given off by heating to 105° or 110° is recorded as H_2O-, and that given off above one of these temperatures is recorded as H_2O+. This represents a decided advance over the older plans of determining only total water, or of drying the sample at 110° before weighing it out for analysis, and for many purposes may be adequate. But our knowledge of hydrous colloidal minerals, and even of many containing water of crystallization, will never be complete unless the actual temperature ranges over which the water is liberated both above and below 110° are recorded. While the boiling point of pure liquid water under a pressure of one atmosphere is 100°, the water held by a solid, even though not chemically combined in the strictest sense, may vaporize at a far lower

[2] Surface tension and similar phenomena may modify this.

[3] LARSEN, E. S., and WELLS, R. C. *Some minerals from the fluorite barite vein near Wagon-Wheel Gap, Colorado.* Proc. Nat. Acad. Sci., **2**: 364. 1916.

temperature than this, and again may be given off only at a much higher temperature.

In the case of the mineral here described the temperature of 100° possesses no significance whatever, as is clearly shown by Table 1, in which the loss at different temperatures is recorded. The coarsely powdered mineral was heated to each temperature in a covered platinum capsule for 2 hours.

TABLE 1

LOSS OF WATER FROM LEVERRIERITE FROM COLORADO

TEMP. °C	PER CENT H_2O LOST		TEMP. °C	PER CENT H_2O LOST	
20	Start		120	0.05	
30	2.05		130	0.05	
40	1.85		140	0.20	
50	2.10		150	0.35	
60	0.75		160	0.25	
70	0.45	Total below 110°, 10.75	170	0.25	
80	1.15	per cent	180	0.35	Total above 110°, 6.65
90	1.60		190	0.30	per cent
100	0.45		200	0.35	
110	0.25		250	0.45	
			300	0.60	
			350±	3.15	
			ign.	0.30	Total water, 17.40 per cent

In interpreting the results it must be borne in mind that the amount of water lost at a given temperature depends on the fineness of grinding, the length of heating, the humidity of the atmosphere in which the heating is done, etc., so that too great significance should not be attached to such data. It is worth noting, however, that over one-third of the total water is given off below 50°, and nearly two-thirds below 110°. Nearly all the water is lost below 350°. By way of contrast it may be mentioned that a sample of halloysite recently studied by us[4] retained 12.9 per cent of H_2O at 400°.

[4] LARSEN, E. S., and WHERRY, E. T. *Halloysite from Colorado.* Journ. Wash. Acad. Sci., **7**: 178. 1917.

Effect of dehydration on the optical properties. In order to determine the effect of dehydration on the crystal structure, samples of coarse powder were examined microscopically after being heated respectively for 8 hours at 65°, 5 hours at 130°, 2 hours at 290°, and 2½ hours at 360°. The loss of so large an amount of water would lead one to expect considerable change in the properties. However, the optical properties remained

TABLE 2

EFFECT OF DEHYDRATION ON THE OPTICAL PROPERTIES OF LEVERRIERITE FROM COLORADO

TEMP. °C.	PER CENT H_2O	β and γ	α	APPARENT[a]		REMARKS
				β and γ	α	
20	17.4	1.602	1.558	1.515	1.470	Indices of refraction of grains immersed in oils appear to increase for several days before reaching the maximum value
75	10.0	1.600	1.555	1.495	1.46	Reaches maximum value in a short time
130	6.5	1.597	1.553	.....	.....	Quickly increases to maximum value
290	3.7	1.600	1.555	.....	.....	Quickly increases to maximum value
360	0.2	1.603	1.559	.....	.....	Quickly increases to maximum value
Redness	0.0	1.54 varies ±0.03	1.50 varies	1.54	.....	Contains clouded areas. Rather deeply colored and pleochroic

[a] The apparent value as measured by the immersion method immediately after imbedding in an oil of that index of refraction.

practically unchanged up to 360°C, and there was only a moderate decrease in the indices after heating for half an hour at redness. The results are presented in Table 2. The mineral remains sensibly uniaxial, even after heating to redness, and the optic axis emerges normal to the plates. On heating, it gradually loses its waxy luster and becomes somewhat darker in color and more strongly pleochroic. It increases in hardness slightly and becomes rather brittle. The perfect cleavage becomes even more prominent than before dehydrating.

No satisfactory interpretation of this remarkable constancy of optical properties with wide difference in H_2O content occurs to the authors. The loss of water of crystallization or of chemically combined H_2O should have a considerable influence on the optical properties and would be expected to be accompanied with a complete change in all the crystal properties. Even the loss

TABLE 3

ANALYSES OF MINERALS OF THE LEVERRIERITE GROUP

	1	2	3	3a	4	5	6	7	8
SiO_2	47.28	47.84	47.56	789	47.95	49.90	50.55	49.4	48.43
Al_2O_3	20.27	20.88	20.57	256	32.67	37.02	19.15	45.1	41.63
Fe_2O_3	8.68	8.48	8.58		0.23	3.65			
MnO		0.24	0.24				4.40		
CaO	2.75	2.52	2.52	89	0.41	tr.	0.63		
MgO	0.70	0.91	0.80		0.46	0.30			2.13
Na_2O	0.97	1.58	1.28		2.47				
K_2O	tr.	tr.			0.24	1.13			
H_2O+	19.72	6.65	6.65		7.03	8.65	24.05	5.6	7.70
H_2O-		10.95	12.01		8.56	?		?	?
Totals	100.37	100.05	100.21		99.36	100.65	98.78	100.1	99.89
$Al_2O_3 : SiO_2$			1 : 2.76		1 : 2.34	1 : 2.11	1 : 3.95	1 : 1.86	1 : 1.94

1 and 2. Material from Beidell, Colorado. New analyses by E. T. W.

3. Average of 1 and 2.

3 a. Molecular proportions of 3.

4. Average of 2 analyses of "rectorite," Garland County, Arkansas. Brackett, R. N., and Williams, J. F., Amer. Journ. Sci., **42**: 16. 1891.

5. Leverrierite, Rochelle, France. Termier, P., Bull. Soc. Min., **22**: 29. 1899. Analysis made on material dried at 110°–130°. Older analyses show 13.21 and 18.0% of total water.

6. "Montmorillonite, var. delanouite," Millac, France. Quoted from Lacroix, A., Mineralogie de la France.

7. "Batchelorite," Tasmania. H_2O stated as "combined H_2O." Gregory, J. W., Trans. Austr. Inst. Min. Eng., **10**: 187, 1905.

8. Kryptotile. Quoted from Dana. The original article reports H_2O without a statement as to whether it represents total water or water above 100°.

of 17.4 per cent of "absorbed" H_2O would be expected to cause a considerable change in the values of the indices of refraction.

Composition. Two different samples shown by microscopic examination to be essentially homogeneous were analyzed by standard methods and yielded the results shown in columns 1

and 2 of Table 3; columns 4 to 8 give analyses of other members of the same group of minerals for comparison. The name leverrierite has been selected for the group because it has priority over all the others except kryptotile, which is, however, only known in indefinite mixtures.

All the minerals shown in the table are essentially silicates of aluminium with some Fe_2O_3 and RO. Assuming that the Fe_2O_3 and RO replace Al_2O_3, the ratio Al_2O_3: SiO_2 varies from 1.86 to 3.95. The H_2O+ and the total H_2O also vary considerably, but no more than might be expected from determinations made under widely different conditions. Kryptotile is reported as having 7.70 per cent of H_2O. If this represents total water, it contains considerably less than the others. Batchelorite, kryptotile, rectorite, and the original leverrierite are near to $Al_2O_3.2SiO_2.2H_2O$ in which about half the H_2O is given off below 110°. The Colorado mineral is nearer to $Al_2O_3.3SiO_2.4H_2O$ and about two-thirds of the water is given off below 110°; while delanouite is near $Al_2O_3.4SiO_2.6H_2O$. However, there appears to be an almost continuous variation of the ratio Al_2O_3: SiO_2 from 1.86 to at least 2.76. This can hardly be due to admixed impurities, as the authors have examined the Colorado mineral and the type rectorite, and have found them essentially homogeneous, and the descriptions of the analyzed leverrierite and batchelorite also indicate homogeneous material, although the delanouite examined by the authors contained some impurity while the description of the analyzed kryptotile indicates impure material. This variableness in composition within definite limits suggests that a mineral group is represented, or at least a species including several subspecies, as is the case, for instance, with muscovite. This view is confirmed by a comparison of the properties of the several minerals in Table 4.

The optical and physical properties of the six minerals, while not identical, are about as nearly alike as are the corresponding data for various specimens of muscovite and other complex minerals; and as in muscovite, the cleavage, optic orientation, optic character, and birefringence show comparatively little variation, while the axial angle and indices of refraction vary

within moderate limits. This similarity, together with the continuous variation in chemical composition, shows with reasonable certainty that all the minerals are members of a continuous

TABLE 4

PROPERTIES OF LEVERRIERITE AND RELATED MINERALS

	RATIO Al_2O_3: SiO_2	OPTICAL ORIENTATION	2v	α	β	γ	REMARKS
Batchelorite	1: 1.86	.....	.....	.....	.....	.....	Foliated
Kryptotile[a]	1: 1.94	X ⊥ cleav.	0–45°	1.558	1.593	1.594	Fibrous, platy
Leverrierite[b] France	1: 2.11	X ⊥ cleav.	0–50°	1.554	1.582	1.582	Vermicular, micaceous
Rectorite[c], Arkansas	1: 2.34	X ⊥ cleav.	38°±	1.550	1.588	1.590	White, dull; tough. Resembles mountain leather. Interwoven fibers and plates. Rather plastic when wet
Material from Colorado	1: 2.76	X ⊥ cleav.	0°	1.558	1.602	1.602	Luster vitreous to waxy, rather brittle. Platy. Very plastic when wet
Delanouite[d]	1: 3.95	X ⊥ plates	Small	1.54	1.570	1.570	Not entirely homogeneous. Feathery plates

[a] New data on specimen from Waldheim, Saxony, kindly furnished by Colonel Roebling. The material closely resembles sericite.

[b] Published data of Wallerant. Other data gives the birefringence as low as 0.008.

[c] New data from U. S. Nat. Mus. specimen No. 80607. The indices vary. When first measured $\alpha = 1.500$, $\beta = 1.541$, $\gamma = 1.543$. The indices appear to increase on standing in oils. (Cf. the Colorado mineral.)

[d] New data on "montmorillonite, Millac." Specimen kindly furnished by Prof. A. Lacroix. A red, clay-like mineral. Under the microscope it is seen to be made up of rather coarse, feathery plates or fibers. The best interference figures show that the mineral is optically negative and has a small axial angle. Some fibers are optically positive and have a large axial angle.

series with varying proportions of SiO_2 and Al_2O_3. This would not require any greater variation in the chemical composition than that due to the change in the water content, which, as was

pointed out above, takes place without a break in the crystal structure, as shown by the optical properties. Muscovite and other micas (to which leverrierite is probably related), show almost as wide a range in the ratio between the SiO_2 and Al_2O_3, as leverrierite; they also show like the latter but small variations in their optical properties.

Leverrierite and some of the minerals here united with it have usually been included in text books on mineralogy as varieties of kaolinite. However, Termier[5] has shown that it is undoubtedly a distinct mineral. He concluded that it is a muscovite in which the K_2O is replaced by H_2O, but since this conclusion is based on an analysis of the mineral dried at 110° it seems hardly justified; yet the cleavage and optical properties are certainly near those of the mica group, to which the leverrierite group is believed to be closely related.

Chemically it differs from kaolinite, chiefly in the fact that it retains only 7 per cent of its H_2O at 110° and very little at 350°, while kaolinite retains nearly all of its 14 per cent of H_2O up to 400 degrees. Optically leverrierite has higher indices of refraction, much stronger birefringence, and much smaller axial angle than kaolinite, and it is commonly found in larger plates.

The leverrierite group includes then, the micaceous hydrous silicates of aluminium with small amounts of Fe_2O_3, RO, and R_2O, in which the ratio Al_2O_3: SiO_2 varies at least from 1.85 to 3.95; the H_2O content under normal conditions is from 15 to 24 per cent, of which all but about 7 per cent is given off below 110°. In physical and optical properties leverrierite resembles muscovite, but its cleavage is less prominent, it is rather brittle when dry and very plastic when wet, and its axial angle is commonly very small.

Summary. Leverrierite occurs in the veins of quartz and manganese oxide at Beidell, Saguache County, Colorado, in cleavage plates up to several inches across. It has a very perfect basal cleavage. It becomes plastic when wet. It is optically negative, practically uniaxial, and the optic axis emerges sensibly normal to the cleavage. The indices of refraction as

[5] TERMIER, P. Bull. Soc. Min., **22**: 27. 1899.

measured by the immersion method, immediately after imbedding in a liquid, appear to be $\alpha = 1.470$, β and $\gamma = 1.515$, but on standing in an immersion medium the liquid slowly penetrates the grains, probably displacing air and possibly some water, and after remaining several days in the liquids β and γ of the grains match a liquid with an index of refraction of 1.602, and α, one with an index of 1.558. These are believed to be the indices of refraction of the mineral. Loss of water, even to the extent of 17 per cent, causes no appreciable change in the optical properties, except a deepening of the color.

A chemical analysis of the mineral is given and the loss of H_2O at different temperatures. This analysis and analyses of rectorite, leverrierite, batchelorite, kryptotile, and delanouite are compared, and show some variation in the water content and, more especially, in the SiO_2: Al_2O_3 ratio, which varies from 1.86 in batchelorite to 3.95 in delanouite. However, optical study of the six minerals indicates that they belong to a single group, probably related to the micas. Analyses of muscovite show almost as wide a range in the SiO_2: Al_2O_3 ratio. The formula of the leverrierite group may be written $Al_2O_3.2 \pm SiO_2.2\frac{1}{2} \pm H_2O$.

MEDICAL ZOOLOGY.—*The carriage of disease by insects.*[1] L. O. HOWARD, Bureau of Entomology.

In his opening remarks the speaker called attention to the fact that the whole great field of the carriage of disease by insects has been developed within the last twenty years. He showed that in the standard medical works of twenty years ago, such as for example the 1895 edition of Osler's *Principles and Practice of Medicine*, there occurs absolutely no mention of insects in connection with the etiology of disease, either of man or of the higher animals; yet at the same time he showed that as early as 1889 Theobald Smith had discovered the causative organism of the so-called Texas fever of cattle (*Babesia bovis*) and that with the experimental aid of F. L. Kilbourne he had

[1] Address as retiring President of the Washington Academy of Sciences, delivered February 1, 1917. Abridged by the author.

shown that this organism was carried from southern cattle to non-immune cattle by the so-called southern cattle tick (*Margaropus annulatus*), the results of this experimental work having been published in 1893.

Even before this, however, Dr. Patrick Manson, now Sir Patrick Manson, had demonstrated the carriage of the parasitic worm, *Filaria nocturna*, responsible for certain of the diseases grouped under the name filariasis, from mosquitoes to man. Manson's discovery was, however, by no means so significant as that of Theobald Smith. The announcement of Smith's discovery, however, coming from a veterinary service and published in the annual report of the Department of Agriculture, unfortunately received little attention from the scientific world in general.

The initial discovery which attracted world-wide attention was that of Ronald Ross in India, who found that malaria is carried by certain mosquitoes.

The speaker here digressed in order to give his views concerning the recently agitated theory of the transmission of infantile paralysis by insects. He said:

> The whole country was interested and alarmed at the occurrence of an unusual number of cases of infantile paralysis during the past summer (23,970 in all, with 2072 deaths out of a total of 7925 cases in New York City alone), and many theories were advanced concerning its method of spread. I must confess that when it was announced that the causative organism had been found in the intestinal passages as well as elsewhere and that it probably enters the body of the patient through the mucous membrane of the mouth and nose, I instantly thought of the house fly and the all too frequent contamination of exposed food by this insect, frequently fresh from intestinal discharges. But a second thought showed me that were such a method of conveyance of the disease possible the disease itself would be much more common and there would have been last summer very many thousands rather than many hundreds of cases. Then too, the not infrequent winter cases could not very well be fly-borne.
>
> Mosquitoes have been suggested as carriers, and a well reasoned paper by Dr. Mark W. Richardson, of Boston, was published last September under the title "The Rat and Infantile Paralysis," the rat-flea of course being the theoretical carrier. But rat-fleas go to human beings only in the event of epidemic disease among rats, and nothing of the sort has been noted in connection with any of the larger epidemics of infantile paralysis.

Moreover in inoculation experiments reported by Flexner and Lewis, the virus is present in the blood of inoculated monkeys in such high dilution that the infection in a normal animal is accomplished only by inoculation of 20 cc. or more of the blood. If this holds under normal conditions, it becomes absurd to accuse any biting insect of the carriage of this particular disease, except in the possible event of the development of the organism in the body of the insect. While this possibility should be studied, the probabilities are against it. The impression which all of us in Doctor Flexner's audience at his lecture given December 28, in New York, gained, was I think that we are still greatly in the dark in regard to this disease, but that possible insect carriage must probably be ruled out.

It was then shown that it is necessary to divide the field under discussion into three categories.

1. Insects as simple carriers of disease, the accidental carriers as it were; that is, insects frequenting places where disease germs are likely to occur and conveying these in their stomachs or on their bodies to food supplies. This is notably illustrated by the house fly.

2. Insects as direct inoculators of disease. These are biting insects which feed upon diseased men or animals and carry the causative organisms on their beaks and insert them into the circulation of healthy animals. In this way anthrax is carried by biting flies; surra is carried in the same way, as is also the nagana or tsetse-fly disease of cattle. So also is bubonic plague carried in this manner by rat-fleas, but here there is more than a passive carriage, as is also the case with the tsetse-fly disease.

3. The third category, and this is perhaps the most important, includes insects as essential hosts of pathogenic organisms. These are the cases in which the parasitic organism undergoes its sexual generation in the body of its insect host and another, non-sexual, generation or generations in its warm-blooded host. To this class belong the malarial mosquitoes, the yellow-fever mosquito, and the rapidly increasing number of species that carry trypanomiases, leishmanioses, spirochaetoses, and the ticks that carry relapsing fevers and other fevers of man and animals, and the lice that carry typhus fever.

Under the first of these three categories the house fly was considered at some length, and cockroaches, ants (especially

the household ant), the latrine fly (*Fannia scalaris*), and other insects were mentioned rather incidentally.

Under the second category the biting flies that carry anthrax were mentioned, and illustrated (as, in fact, was the entire address) by lantern slides. Under this head also, carriage of bubonic plague by rat-fleas was discussed at some length.

Under the third category, insects as essential hosts of pathogenic organisms, attention was called to certain tapeworms which have alternate hosts in insects or other arthropods and domestic animals, especial mention being made of *Hymenolepis diminuata* which lives commonly in the intestines of rats and mice and has as its alternate hosts certain insects which feed in meal, so that man may become affected by eating dejecta of such insects in dirty cereals. The carriage of *Filaria nocturna* by *Culex fatigans* (*quinquefasciatus*), and Ransom's discovery of the house fly parasite *Habronema muscae* as a stomach parasite of the horse, and the pig parasite (*Echinorynchus gigas*) sometimes occurring in man, with its alternate hosts as the larvae of cockchafers in Europe and the common white grubs (larvae of Lachnosterna) in the United States, were described.

Then followed a longer consideration of mosquitoes and malaria, and mosquitoes and yellow fever.

Under the head of trypanomiases, the carriage of the nagana of African cattle by *Glossina morsitans* and the sleeping sickness of Africa carried by *Glossina palpalis* were mentioned, as well as the wasting disease of children in Brazil known as opilacao, caused by *Trypanosoma cruzii* and possessing a definitive host in the large biting true bug *Conorhinus megistus*

Then followed a consideration of insects and leishmanioses, ticks and spirochaetoses, including some detailed account of ticks and the Rocky Mountain spotted fever. A fuller consideration was given to typhus fever and lice.

Stating that the carriage of typhus fever by the body-louse was first demonstrated by Ricketts in the City of Mexico, where this discoverer lost his life from this fever in the course of his investigations, mention was made of the tremendous death rate from this disease during the last Balkan war in Serbia and

its destructive appearance in many p'aces during the present great war. It was shown that at first the information put out by the medical departments of the different armies was insufficient and in many cases illy based. Especial mention was made of the publications issued in England, France, and Germany, the extraordinarily detailed observations by Haase, made in Germany in the camps of Russian prisoners, receiving special consideration. He showed that more recently an intense investigation has been carried on in many places of all of the aspects of the biology of the body-louse. He showed that in the current number of the *Bulletin of the Pasteur Institute of Paris* (December 15, 1916) reviews had been found of seventeen papers, under the heading La Lutte Contre les Puces. One of these was written by a Japanese, four by Englishmen, seven by Germans, one by a Swiss, two by Frenchmen, one by a Russian, and one by an Italian. He pointed out especially the very perfect proof adduced in one of these articles of the transportation by wind of the body-louse, a very important point to be considered in sanitary measures.

In concluding, the speaker referred to a manuscript table drawn up by Mr. W. D. Pierce from the recent iterature, which indicates that discoveries have been recorded of 226 different disease organisms as carried by insects to man or animals; that 87 organisms are known to be parasitic in insects but not known to be transmitted, and that 282 species of insects are recorded as causers or carriers of diseases of man or animals.

The concluding paragraphs of the address are quoted:

> But now we must stop. There are many subjects in the field which we have not touched. Tick paralysis, for example, is a most interesting and novel subject. This disease occurs in Australia, Africa, and North America. In Oregon thirteen cases have been found in the practice of a single physician. The attachment of a tick brings about progressive paralysis involving motor but not sensory nerves. It seems a unique malady. Hadwen and Nuttall, showing that it is not infectious and that there is apparently an incubating period in the tick, suggest a specific causative organism, but others hold to the theory of nerve shock.
>
> Attention should also be called to the fact that, in spite of the host of discoveries already well established, there is a dangerous tendency

to exaggerate the importance of insect transmission, and to overlook, even in cases where insects may occasionally be concerned, the greater importance of other modes of infection. This is indicated by Sambon's theory of transmission of pellagra by Simulium—a theory which was advanced with enthusiasm on the ground that it fitted into the known facts in the epidemiology of the disease. It took two years of hard work on the part of members of the force of the Bureau of Entomology, working in collaboration with the Thompson-McFadden Pellagra Commission, to upset this theory in a thoroughly scientific manner. As has been pointed out several times of late, there is always considerable danger in conclusions based on epidemiological findings. Transmission experiments are necessary.

One conclusion must be drawn which can hardly be disputed: There is an enormous field for the entomologist in the careful study of all of the aspects of the biology of not only those insects which have already been shown to be disease carriers but of those which are likely to be implicated. It is to the trained economic entomologist that we must look for the methods of destruction of these insect carriers, and the prevention of this class of diseases lies at his door rather than at that of the physician. Either that, or sanitarians must be trained in what is now known as medical entomology.

ETHNOLOGY.—*Remarks on American Indian languages, a study in method.*[1] TRUMAN MICHELSON, Bureau of American Ethnology.

At the very beginning of this subject it should be stated that there is no single type of speech which holds good for all American Indian languages. The statement that all American Indian languages are both polysynthetic and incorporative, so confidently affirmed by the older writers, is false. The number of American Indian languages that are either polysynthetic or incorporative, is extremely limited indeed. I do not know of a single feature that may be said to be characteristic of all American Indian languages. Even so, a combination of certain features is quite sufficient to determine whether any given language is an American Indian language or not. It is this which enables us to say without any hesitation that Chuckchee, Koryak, and Yukaghir (which are spoken in northeastern Asia) are Americanoid languages. If they were spoken in America we would call them American Indian languages. They do not belong geneti-

[1] Printed with the permission of the Secretary of the Smithsonian Institution.

cally (as far as is known) to any American linguistic stocks, but nevertheless the totality of their features compels us to classify them with American Indian languages. To account for these facts we must assume either that these tribes are the descendants of the forefathers of American Indians who did not follow their brethren in the migration from Asia to America (for such a migration is firmly established by the facts of physical anthropology), or that there has been in comparatively recent times a migration backward from this continent to Asia. In as much as the bulk of American Indian languages are spoken on this continent, from a purely linguistic point of view the latter hypothesis is the most probable. But the essential fact in any case will remain unchallenged, namely, that we have American-oid languages spoken in northeastern Asia.

Let us now turn to the almost unparalleled number of linguistic stocks on this continent. We have an apparent anomaly as compared with most parts of the world. The point at issue is whether such a multiplicity of stocks is original or not. In the first place, in the study of American Indian languages we are at a decided disadvantage as compared with the study, say, of Indo-European languages. Suppose that modern English and modern Russian were the sole survivors of the entire stock. It would be impossible to prove absolutely that they were both genetically descended from a common ancestor, no matter what we might surmise. It is only because we have continuous written records of both covering several centuries, and have the aid of other related languages which have even earlier records, that we can absolutely prove this. For this reason it is clear that there always will be American Indian languages whose genetic connection we may suspect, but which we can not prove. It may be urged that we can actually see what has taken place in the development and differentiation of languages which have been historically transmitted, such as Indo-European languages, and that we should apply the principles derived from such a study to American Indian languages in determining the stocks. The methodical error in such a procedure lies in this, namely, that there are less than a half a dozen, different stocks in

the entire world of which we have records going back continuously for more than a thousand years. The percentage of the total stocks so transmitted is altogether too small to afford a firm and sure foundation for such a mode of action. If we could establish from a minute study of the dialects of some dozens of stocks that the kind of differentiation, etc., was on the whole of a similar nature in these stocks, we would be entirely justified in applying the principles derived from such a study to the determination of the limits of stocks in American Indian languages and other stocks as well. Unfortunately such a study has not been made, nor is there any prospect of it being done in the immediate future.

We have a similar difficulty in the reconstruction of parent-languages of American Indian linguistic stocks. In the case of Indo-European languages we again can take advantage of principles derived from a study of the historical development of the separate members of the stock, and apply the results to the prehistoric period. We can not do this in the case of American Indian languages. The nearest approach to this would be a very minute study of the dialects of known stocks. In some cases there is no doubt that this would even largely counterbalance the difficulty spoken of. For example, most of the dialects of the Algonquian stock are so closely related that it can readily be ascertained in at least many cases what is archaic and what is secondary. Thus it is certain that the Fox *e* and *i* vowels are more primitive than the Ojibwa *i* vowel, and that the terminal vowels preserved in Fox, Sauk, Kickapoo, Shawnee, and Peoria, but not appearing in Ojibwa, Ottawa, Potawatomi, etc., are archiac.[2] Hence these features are to be ascribed to the Algonquian parent language. However, we can not know that precise quality of the prehistoric *e* and *i* vowels. Similarly the combination of a sibilant followed by a surd stop in Cree is more archaic than the correspondents in many of the related languages, and so is to be likewise ascribed to the parent language. (The actual proof that the Cree combination is more

[2] Amer. Anthrop. N. S., **15**: 470. 1913; this JOURNAL, **4**: 403. 1914; Ann. Rep. Bur. Amer. Ethnol., **28**: 247. 1912.

archaic is too complicated to be given here, as it would of necessity be altogether disproportionate to the length of this paper.) Naturally, in some instances, absolute proof would be wanting. Thus in certain cases Ojibwa *nd* corresponds to Cree and Menominee *'t*, Fox *t*. It can be easily shown that the Fox *t* in this case is in all probability unoriginal; and most philologists will assume that Ojibwa *nd* is more archaic than Cree and Menominee *'t*. But probability and assumption are not the same as proof.

A further obstacle to the reconstruction of the parent languages of American Indian stocks is our frequent inability to formulate phonetic laws in a manner such as is demanded by all modern Indo-European philologists. These contend that phonetic laws in themselves admit of no exceptions; and that wherever we find apparent exceptions, there is some extraneous reason, or reasons. The historical study of the individual Indo-European languages shows that analogy and the like have been potent factors in transforming them, and are fully as important as the actions of phonetic laws. For example late Latin *potēbam* is not a phonetic transformation of Latin *poteram*, but is due to the influence of other imperfects in *bam* preceded by a long vowel. For this reason we are justified in extending the principles derived in this manner to the prehistoric period, to harmonize discrepancies among the historical languages which cannot be accounted for by phonetic laws. Thus the Italic languages have an ablative singular of *ā* stems in *ād* (retained in Oscan and early Latin; final *d* lost in classical Latin by phonetic law). The collective study of Indo-European languages shows conclusively that the ablative singular of *ā* stems was the same in form as the genitive. Since the same study demonstrates that *o* stems in the Indo-European parent language had an ablative singular in *ōd* (preserved in early Latin; *d* lost phonetically in classical Latin), and that no other stems in the Indo-European parent language had a special case form for the ablative singular; and since we know that in historical Indo-European languages analogy has been a potent transforming factor, we have an entirely legitimate right to assume that the Italic languages developed an *ād* abla-

tive singular for *ā* stems by the influence of the *ōd* ablative singular of *o* stems. In one sense the proof is not absolute, but it is as absolute as it is possible to give when dealing with prehistoric phenomena. It must be admitted that at times even Indo-European philology is at sea, and that pure'y subjective speculation may come into play. Happily these instances are rare. These remarks are inserted because although the facts are well-known to Indo-Europeanists, they are largely unknown to Americanists.

The bearing the above has on the problems of American linguistics is this: since American Indian languages have not been transmitted to us in the manner that Indo-European languages have, we do not know what has disturbed phonetic laws in many given cases, and for the methodical reasons outlined above, we are not justified in assuming that the same influences have been at work in American Indian languages as in Indo-European languages. Herein lies our difficulty in formulating phonetic laws that are entirely satisfactory to the Indo-Europeanist. For example, *n* becomes *c* in Fox before *i* which is a new morphological element; it remains if the *i* is not such an element. There are some specific grammatical categories in which the law does not work. A study of several related dialects shows that this change also takes place in them, and hence must be very old. At the same time the apparent exceptions have not been explained. Whether they ever will be, is questionable. Yet an Americanist does not object to the formulation of the law as it works in practice. The Indo-Europeanist will object vigorously to such a formulation as it is contrary to his accepted canons. If the canons of Indo-European philologists be accepted, it is quite evident Americanists can not reconstruct the parent languages of American Indian stocks in an entirely satisfactory manner. However, most Americanists are far more interested in observing actual phonetic correspondences and the like between the different dialects of linguistic stocks as they actually occur, than in speculations which from the nature of the case must rest upon rather slim foundations. It may also be noted in this connection that Indo-Europeanists have begun to interest

themselves more with the linguistic problems of historical languages, and less with the remote Indo-European parent language.

Let us return again to the question of the multiplicity of American stocks. As stated above, this is today almost without parallel. However it does not follow that this has always been the case. In Europe we know definitely that Etruscan has been wiped out; but we do not know how many distinct stocks were obliterated by the spread of Indo-European languages. It is entirely possible that many have been so obliterated. If they have, we have then a case quite analogous to the situation in America. But this is merely speculation. The problem may be approached from a different point of view. There is no reason to suppose that the migration from Asia was all from a single stock, in other words, that the differentiation has all been on American soil. Were that the case, in spite of the enormous lapse of time, surely we would be able to find at least one striking morphological trait common to all American Indian languages, for the morphology of a language is its most permanent feature.

Though, as intimated above, we have an apparent multiplicity of stocks which can not be reduced, nevertheless a number have resemblances to each other. An example is Siouan and Muskogean. The question resolves itself to this: Are these resemblances indicative of a common origin so remote that it is no longer possible absolutely to prove it, or are such similarities due to borrowings? To settle the question we must know what may be borrowed. That sounds may be borrowed across extremely divergent linguistic stocks is abundantly proved by the languages of the Northwest coast where we have the condition that languages whose morphology and vocabulary are distinct have practically the same phonetic elements. That vocabulary may be borrowed across linguistic stocks is too well-known to require illustration. That syntax may be borrowed across linguistic stocks is shown by the languages of Mexico where Spanish syntax has patently influenced that of American Indian languages. At this point we may ask a question, namely, can morphological features be borrowed? This is one of the most pressing problems of linguistic science awaiting solution. Unfortunately we have

little material at hand definitely to prove or disprove it. Such as we have tends strongly to establish it. For example it is patent that the post-positions of Wishram (Chinookan stock) are due to the influence of Sahaptian, a distinct though contiguous stock. Similarly, classifiers are common to the Salishan, Wakashan, Chimmesyan, Koluschan, and Skittagetan stocks which are at the same time contiguous. One may suspect, indeed, that the first pair, and similarly the last pair, have differentiated from a common ancestor. Yet at the very best we would have a single striking morphological trait spread throughout three stocks which otherwise have nothing in common in either morphology or vocabulary, but only have resemblances in sounds. It is too great a strain on the imagination to believe that this is wholly the result of chance. If the accepted definition of stock is to remain, namely, a stock consist of one or more languages all of whose sounds, morphology, syntax, and vocabulary genetically have descended from a single ancestor, we must admit at once that this trait which is held in common, is due to borrowing. For the differences in vocabulary and morphology are so enormous that it is inconceivable that the present differences are solely due to later differentiation. The vocabulary and morphology which cannot be explained at present are just as important as the extremely small percentage that can. There are a few other cases in which morphological borrowings between stocks is plausible, but they are entirely too few in number to warrant us at present in applying the principle broadcast. So we have to content ourselves in the meanwhile in pointing out structural resemblances between stocks, such as between Esquimoan and Algonquian, which resemble each other strikingly in their pronominal systems, and to a much less extent in the building up of verbal stems, in the hope that a careful and minute study of all the dialects of such stocks may enable us definitely to affirm whether such traits are due to early differentiation from a common remote ancestor or to comparatively recent borrowings.

It must be admitted that recently there has been a decided tendency among Americanists to consolidate such stocks as

show only a moderate amount of common lexical and morphological resemblances. In some cases, such as Uto-Aztecan, it must be conceded that the burden of proof is now definitely on those who maintain that the two "stocks" are true distinct stocks, and not differentiations from a common ancestor. It is on the other hand equally certain that the genetic connection has not been established with absolute nicety. In the same manner Athapascan, Koluschan, and Skittagetan are almost certainly genetically related. Their morphological resemblances are so numerous and so special that it is incredible that they are due solely to borrowings, and not to genetic relationship. However, the amount of lexical material the three have in common is an exceedingly small percentage of their total vocabulary. The extremely large percentage of the unexplained lexical material forces us to admit that this has been derived from outside sources, and with our present definition of "stock," a purely genetic relationship between the three breaks down. After all, our difficulties all hinge on our definition of "stock," and the proofs necessary to show that one or more languages constitute such a "stock." Though the definition of "stock" given above, may be rigorously correct, the actual application of it would practically obliterate the total number of "stocks" in the world, and we should be worse off than ever. For in that sense, there are few, if any, languages which constitute a stock. In the writer's opinion philologists have taken over biologists' concepts without inquiring whether they are suitable to their own science. "Stock" must be redefined in a way that has some real meaning, and some term or terms invented to cover those larger groups which apparently are only remotely related, which may be related in sounds, morphology, syntax, and vocabulary, but not all combined as a unit. It goes without saying that the nature of the proofs then demanded will be in accordance with our definitions.

To revert to a point brought out above. If the morphology of languages can not be borrowed, with scrupulous nicety we must assume an enormous number of distinct stocks at the very dawn of man, which certainly is not plausible. If on the other

hand, sounds, morphology, syntax, and vocabulary have no innate connection (vide supra) then we would have a series of various borrowings instead of "stock" in its present sense. It may be well however, to state that no matter how "stock" is defined at present, it certainly is not used in that sense alone, but in a more loose way, almost according to the whim of the author.

The most decisive proof that two or more languages belong to the same stock, used in a somewhat free sense, is numerous and detailed resemblances in their structures. A large percentage of vocabulary held in common is a welcome additional proof. The most decisive proof that a single language constitutes a special stock is numerous unique morphological features. No amount of purely lexical resemblances between languages, no matter how far apart geographically, would prove that they belonged to a single stock. For, experience has shown us that vocabulary is very often borrowed in large amounts, and hence is not a good criterion. If the tribes were far apart geographically, that would not preclude the possibility that in prehistoric times they had been in contact, and at that time extensive borrowing had taken place. Another reason why vocabulary is not a good criterion is that the number of words in even distinct stocks that superficially resemble each other is really considerable. An example is Sanskrit (Indo-European stock) *ásan-*, Fox (Algonquian stock) *asen·i*, both meaning "stone." Comparisons of vocabulary are only valuable when we know that the morphology of the languages compared are the same, or at least very similar. Otherwise we should not know whether we were dealing with comparable elements, even if the words in their totality resembled each other. For example, the comparison of the pronoun Avestan *čiš*, Greek *τίς*, Latin *quis*, Oscan *pis*, all meaning "who," is entirely justifiable, because the structure of all four languages is fundamentally the same; and, which is also important, it has been shown that though these words apparently resemble each other only slightly, yet as a matter of fact the correspondence of the various sounds forming these words is precisely what we should expect from our knowledge

of comparative Indo-European phonology. Another example of a case where words of two different stocks superficially resemble each other is Sanskrit *naṣṭa-* "dead," Fox *ne'tōw*^{a} "he kills." It will be recalled that the combination of a sibilant followed by *t* appears as *'t* in Fox. As soon as we note that Sanskrit *naṣṭa-* is composed of the elements *naṣ+ṭa-* and Fox *ne'tōw*^{a} of the elements *ne+'tō+w*^{a}, the comparison ceases to interest us. [The Fox word can, as I think, be reduced still further in analysis, but this only still further emphasizes the point at issue.] We should exercise the same prudence in comparing morphological elements. For example the verbal termination of Latin in the third person singular is *t* which superficially resembles Tsimshian *t*, to say nothing of similar terminations in various Algonquian dialects. Again Greek *μεν*, the verbal termination of the first person plural, has an entirely fortuitous resemblance to Ojibwa *min* in *ni—min*, *ki—min* of the independent mode.

Turning now to the classification of the languages belonging to single stocks,—it must be said that very little work has been done on this important topic in American linguistics. The three stocks of which we have the best knowledge in this respect are Salishan, Siouan, and Algonquian. To a certain extent the classification is arbitrary. We select a number of salient features and base our classification on it. In most cases we have overlappings which are indicative of more than one association. For example, Peoria fundamentally belongs with the Ojibwa division of Central Algonquian languages; at the same time there are certain traits which clearly prove that it has also had an association with the Sauk, Fox, Kickapoo group, also one with Cree; and there are some indications of contact with Delaware-Munsee. Nevertheless in spite of such short comings, we can make classifications which are entirely satisfactory even to the Indo-European philologist. The object of our classifications is to determine the prehistory of the tribes of any given stock. For example, the Abnaki dialects exhibit so many special traits in common with Shawnee, as wel as Sauk, Fox, Kickapoo, that it is absolutely certain that in prehistoric times the tribes

speaking these dialects have been in long and intimate contact. In the same way the Central Algonquian dialects are all so closely related, that it is evident that the place of diffusion must have been of limited area. The present geographical distribution of the tribes is insignificant in comparison with the evidence obtained by linguistic procedure. Nevertheless it has its value. It can not be accident that the most divergent Algonquian languages are spoken at the extreme western boundary of the stock.

This last brings us to the question of whether the differentiation of a stock into various dialects is due to evolution without external influence or whether it is due to linguistic shock. To this we reply that every case must be judged on its own merits. Peoria, though spoken today by only a handful of people, has been and is, exposed to far greater linguistic shock than Ojibwa which is spoken by several thousand persons; and, notwithstanding, is in many essential points a far more archaic language than Ojibwa. At the same time its diverse affinities indicate a series of shocks. Similarly, it can hardly be doubted that the divergent character of Aleutian (Esquimauan stock) is due to such shock. It should be candidly admitted that our studies in American linguistics have not yet reached a point where these problems can be answered in a thoroughly satisfactory manner.

It will be remembered that we are not in the position of the Indo-European philologist who can observe what has happened in the differentiation of dialects in a space of several centuries, and thereby draw legitimate inferences as to what happened in the prehistoric period. We also lack good time-measures; so we can not tell how long it has taken such and such a dialect to differentiate itself from its kindred. In this connection it should be stated that the Indian words cited by early travellers and missionaries are so badly recorded, that if the language from which the word is cited, is still extant, ordinarily we can do far better with the words spoken today. The methodical error of using the analogy of Indo-European languages in solving the points at issue, has been shown above. Our guesses from such inferences might be very happy, but they would remain guesses, not proved

facts. Our whole endeavor should be to place American linguistics on a rigid scientific basis, not upon a foundation of guesses. If I have spoken strongly on this point it is because some of the most eminent Americanists apparently desire to do the latter. They wish, without sufficient reserve, to consolidate stocks that apparently resemble each other, and are content to leave it to the next man to prove or disprove their cases. I do not deny that very likely they have made some extremely happy guesses, but I do deny that the *evidence thus far produced*, has been of such a nature as to compel acquiescence in all the results they claim to have obtained.

The opponents of the views here expressed will reply that the proofs demanded are more suitable to the so-called exact sciences than they are to philology; that if such a program be adhered to, we can not possibly hope to accomplish our task, as the languages are rapidly disappearing; that they are interested only in great facts, not minutiae; and that Indo-European philologists concern themselves largely with the latter simply because the great facts of Indo-European philology are known, and hence there is nothing else left for them to busy themselves with. To all of which the following rejoinder may be made: The proofs demanded may possibly be more rigid than those the Indo-Europeanists ordinarily demand, but it should be noted that every year the proofs demanded by the latter are becoming more and more rigorous, so that the disparity is after all being rapidly reduced, and in time doubtless the proofs demanded by philology will be fully comparable to those demanded by the exact sciences; that there is no advantage in erecting a magnificent edifice on such a weak foundation that it may topple over any minute; it is better to do even a little, and that portion well; and lastly, though it may be granted that Indo-Europeanists today are primarily concerned with minutiae, the discoveries of Grimm, Grassmann, Verner, Brugmann, Collitz, Schmidt, Ascoli, de Saussure—all of which were due to more rigorous methods—are of capital importance; and lastly it should be observed that centuries of quasi-scientific study of languages preceded the discovery of the genetic relationship of Indo-European languages,

which of course will be counted as the great fact of Indo-European philology.

To change to a different topic. The study of American Indian languages will show that there is no such thing as superiority in language. They all possess the necessary machinery to express the most complicated ideas. The point is simply that what is grammar in one language may be vocabulary in another, and vice versa. For example there are no prepositions in Algonquian languages, but what we call prepositions are expressed by grammatical processes or special features in vocabulary. Thus "on" will be expressed by the locative case. "To come in" in Fox is *pīti*, "to go out" is *nōwi*. Nor is the language of primitive peoples indicative of low mentality. Thus Fox has but two fractions, one-half and one-quarter. It does not follow at all that the Fox Indians can not conceive of other fractions. As a matter of fact they can. The point is that ordinarily their life is such that there is no necessity for expressing them. I have tested this again and again by asking interpreters how to say, "Give me a third of that pie," and the like. In every case they were able without the slightest hesitation to render the idea, though not the precise words. I mention this simply because Gobineau apparently still has a goodly number of followers and admirers.

In conclusion, the study of American Indian languages is an extremely attractive field for students of general linguistics; and one that as yet is almost virgin soil. By their study the Indo-European specialist will find his scope vastly broadened, especially in the so-called philosophical bearings of his science. The American ethnologist who neglects their study, places himself wholly at the mercy of interpreters; or at the best must rely on the ipse dixit of his wiser colleagues. For these reasons their scientific study should be fostered in every possible way.

ABSTRACTS

Authors of scientific papers are requested to see that abstracts, preferably prepared and signed by themselves, are forwarded promptly to the editors. Each of the scientific bureaus in Washington has a representative authorized to forward such material to this journal and abstracts of official publications should be transmitted through the representative of the bureau in which they originate. The abstracts should conform in length and general style to those appearing in this issue.

GEOLOGY.—*Structure of the Vicksburg-Jackson area, Mississippi.* OLIVER B. HOPKINS. U. S. Geological Survey Bulletin 641–D. Pp. 93–120, with 1 plate. 1916.

The object of this report is to show which of the areas in west-central Mississippi that were examined by the Survey are considered favorable and which unfavorable for the occurrence of oil, to discourage drilling in the unfavorable localities, and thus to aid those interested in making conclusive tests to determine the presence or absence of oil and gas.

All the rocks of the area are sedimentary in origin and are relatively young, the exposed rocks ranging in age from Claiborne (Eocene) to Recent. Of these formations the loess and the Jackson underlie by far the greater part of the area, and the Vicksburg and Catahoula formations and the terrace sand and gravel underlie smaller areas.

The general structure of the Gulf coastal plain is simple. A series of beds slopes gently southward and passes successively deeper and deeper beneath more recent deposits toward the coast. This general dip toward the coast is interrupted by local steepening or flattening and in a few places by a reversal in direction. These irregularities of dip, which are of greatest significance in the accumulation of oil and gas in valuable pools, are well illustrated in the Vicksburg-Jackson area. The geologic structure is represented on a map by contours on the Vicksburg limestone. The possibilities of oil and gas occurring in the area are dicussed and the most promising areas for prospecting are pointed out. R. W. S.

PROCEEDINGS OF THE ACADEMY AND AFFILIATED SOCIETIES

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

The 785th Meeting was held at the Cosmos Club, March 3, 1917. Vice President HUMPHREYS in the chair; 49 persons present. The minutes of the 784th meeting were read in abstract and approved.

By invitation, Mr. A. H. PFUND presented an illustrated communication on, *The colors of mother-of-pearl.* At the request of the U. S. Bureau of Fisheries the following work was undertaken to ascertain the cause of the iridescence of mother-of-pearl. While this subject has been studied before, the results have been largely of a qualitative nature. By extending the observations into the infra-red region of the spectrum where conditions are much simplified, the author has succeeded in obtaining quantitative measurements which substantiate the explanation of the colors of mother-of-pearl as given by Sir David Brewster.

Under the microscope, a thin section of the shell of a fresh-water mussel is seen to consist of three layers—the inner one being the true mother-of-pearl (nacreous matter). This, in turn, consists of innumerable thin layers of $CaCO_3$ separated by extremely thin layers of organic matter. As the shell grows, successive layers of nacreous matter are deposited on the inside of the shell—each layer projecting slightly beyond the terminal edge of its predecessor. Hence the outside of the nacreous layer will present closely spaced steps or ridges running parallel to the growing edge of the shell. Such a surface structure is identical with that of a diffraction grating, and hence diffraction colors ought to exist. By laying bare the outer surface of the nacreous matter and depositing an opaque layer of silver on it, striking diffraction colors are seen. This surface structure may be studied most readily by means of celluloid casts or replicas of the surface. These replicas show brilliant diffraction colors. Under the microscope the individual lines may be seen and the spacing may be determined. While this is subject to wide variations, the range of spacing observed on the outside of the shell of a fresh-water mussel ranges from 6000 to 25,000 lines per inch. As might be expected, no trace of such a grating-like structure is obtained from the inside of the shell.

As a rule, the diffraction colors are rather inconspicuous, being masked by the blaze of colored light from underlying regions. Since this light is due to reflections from the numerous laminae and since these are parallel and sensibly equi-distant, coloration due to interference might be expected. According to the usual theory, the optical

retardation of two beams reflected from two successive layers is $2\mu e \cos r$ where e is the thickness of the layer, μ the refractive index and r the angle of incidence. If this retardation is a whole number of wave-lengths, constructive interference will take place. Hence maxima of intensity will exist when

$$2\mu e \cos r = \lambda_1, 2\lambda_2, 3\lambda_3$$

etc., where $$\lambda_2 = \frac{\lambda_1}{2} \text{ and } \lambda_3 = \frac{\lambda_1}{3}$$

Such maxima were looked for by reflecting white light from the polished surface of nacreous matter and by measuring the intensities in the various spectral regions by means of a spectrometer and vacuum thermo-couple. Since the interference maxima are most widely separated and are sharpest in the infra-red, the observations were carried out in this spectral region. Results obtained from brilliantly colored specimens were entirely in agreement with the above theory, i.e., the wave-lengths of the interference maxima were in the ratio $1:\frac{1}{2}:\frac{1}{3}$. This quantitative determination proves definitely that the brilliant iridescence of mother-of-pearl is due to interference.

By determining the refractive index of the nacreous matter, it was found possible to determine the actual thickness of the laminae. The results, which differed somewhat for different specimens, showed that the thickness is of the order 0.4–0.6μ.

Discussion. The paper was discussed by Messrs. Sosman and Wright. Mr. Pfund was of the opinion that the number of layers of material in the shells examined was too great to correspond with the number of days or with the number of tides occurring during the life of the shell.

Mr. W. F. Meggers then presented an illustrated paper on *Interference measurements of wave-lengths, and infra-red spectrum photography.* Interference measurements of wave-lengths in the portion of the iron arc spectrum in which the International Secondary standards exist (3233 A to 6750 A) have been made at the Bureau of Standards. The wave-lengths of about 320 lines were compared with those of the 85 secondary standards by the interferometer method of Fabry and Pérot. The average interval between standards was thus reduced from 40 A to 8 A, making accurate interpolation easier. Whereas 86 per cent of the secondary standards have intensities 4 to 6 inclusive, only 47 per cent of the new lines have these intensities, the remainder being distributed among fainter and stronger lines. This makes it possible to photograph sufficient standards with a wider latitude of exposure.

Similar measurements were made in the spectra of the rare gases, helium, neon, argon, krypton, and xenon. The wave-lengths were compared with the fundamental spectroscopic standard (wave-length of red radiation from cadmium = 6438.4696 A). The measurements in the neon spectrum show differences in the numbers of waves per

centimeter (frequency differences) to be constant to 1 part in several millions.

An extensive application of these standardized wave-lengths has been made to an investigation of spectra in the red and adjacent infra-red regions. The arc spectra of 20 of the chemical elements have been photographed from 6000 A to 9000 A and beyond. The photographs were made in the first order spectrum of a concave grating of 640 cm. radius. Ordinary photographic plates stained in dicyanin solution were used and found to be quite sensitive to the long waves. Wave-length measurements on the international scale have been made in these spectra and most of the frequency differences in the spectra of the alkali metals are shown to be constant to 1 part in several hundred thousand of the wave number.

Mr. F. E. FOWLE presented an illustrated paper on *Spectroscopic field light.* When a spectrum is formed the energy observed at any wave-length is not simply what is proper to that wave-length but it is diminished by a portion scattered into other parts of the spectrum

TABLE 1

SPECTROSCOPIC FIELD LIGHT

1. Wave-length in μ	1.8	5.7	7.0	8.0	10.0	12.0	14.0	17.5
2. Deviation in minutes	0	10	15	20	30	45	60	100
3. Quartz transmits	85,000	206	117	85	50	27	17	8
4. Ditto corrected	100,000	242	141	100	59	30	20	10
5. Total field light		244	146	107	66	33	22	11
6. Black-body radiation	100,000	5000	1800	1000	400	140	65	20
7. Nernst-lamp radiation	100,000	5000	1800	1000	400	140	50	2

and increased by portions scattered in from other regions. In measurements in long-wave spectra this scattered radiation or field light may become very important and troublesome. Rock-salt prismatic energy curves were made of the radiation from a Nernst lamp with a half-centimeter thick quartz plate inserted between the energy source and the silt of the spectroscope. The quartz is nearly transparent for light waves less than 4μ in length, and opaque for those longer. The resulting energy curves consisted of two parts of quite different significance: 1st, a nearly symmetrical sharp maximum, the energy curve of the source to 4μ; 2nd, beyond this the energy curve of the light scattered from this region into that for which quartz is opaque. This central maximum with a wing-like appendage on each side was assumed to represent closely the energy curve of a monochromatic line with the radiation scattered to each side in the spectrum. This curve was used to compute the energy scattered into and away from each region of the spectrum. A summary of the results is given in Table 1 in which the captions in the first column have the following significance:

(1) Wave-lengths in millionths of a meter μ; (2) Deviation differences in minutes of arc, 15° rock-salt spectrum, zero deviation at 1.8μ;

(3) Nernst-lamp spectrum observed through a ½ cm. quartz plate; (4) Ditto corrected to allow for the reflections from the surfaces of the quartz plate; nearly equivalent to ordinates of the energy curve of a monochromatic line with the energy scattered to the long-wave side; (5) Total field light which would be expected in the spectrum of a Nernst lamp when the intensity of the latter is 100,000 at 1.8μ (see line 7); (6) Computed relative intensities in black-body spectrum of a body at 2200°K radiating to one at 300°K; (7) Corrected observed intensities of Nernst-lamp spectrum approximating the conditions of line 6 but differing for wave-lengths greater than 12μ because of the increasing absorption of energy in the rock-salt plate closing the vacuum bolometer case and in the prism, and the decreasing absorption by the lamp-blacked surface of the bolometer strip. The intensity of the scattered energy is really very small in any given region although the total amount lost from any region amounts to about 3 per cent of the true intensity of the region. At an angle of 10 minutes from the direction in which the beam is normally reflected the intensity of the scattered energy is only about 0.2 of 1 per cent of that of the main image. It will be noted in the table that at 17.5μ the field light is over five times the true-energy in the lamp spectrum. From a badly tarnished mirror the increase in scattered light is relatively greater for greater deviations from the main image. (This paper will probably appear in the Astrophysical Journal.)

Discussion. Mr. C. G. ABBOT commented on the large amount of work necessary to obtain the results presented.

Informal communications. Mr. C. G. ABBOT spoke of difficulties in producing satisfactory damping in galvanometers, and Mr. W. P. WHITE mentioned a variation in damping with the magnitude of the deflection, that would seem to be due to the damping effect of the air in the clearance space between the armature and the pole pieces.

DONALD H. SWEET, *Secretary.*

THE BIOLOGICAL SOCIETY OF WASHINGTON

The 567th regular meeting of the Society was held in the Assembly Hall of the Cosmos Club, Saturday March 10, 1917; called to order by President HAY at 8 p.m.; 45 persons in attendance.

On recommendation of the Council, Mrs. L. O. HOWARD and Dr. MARTHA BREWER LYON were elected to active membership.

Under the heading book notices, brief notes, etc., Dr. H. M. SMITH exhibited a manuscript and hand-illustrated book dealing with beetles. It was about 60 years old. Dr. Smith presented it to Dr. L. O. Howard. Prof. W. P. HAY presented some notes on the flying squirrels of this vicinity with observations on their habits and behavior as pets. Dr. H. E. AMES called attention to a newspaper clipping recording the flight of two tagged ducks a distance of 2000 miles in about 60 hours. He sought verification of the statement.

The regular program consisted of two communications:

M. W. LYON, JR.: *Precipitins.* Dr. Lyon described an antibeef-serum he had lately prepared, and set up a series of test tubes containing dilutions of beef, sheep, hog, and human serums. He demonstrated the action of the antibeef-serum on these, the specific precipitation when added to the diluted beef serum, the group precipitation with diluted sheep serum, and the non-precipitation with diluted hog and human serums. He mentioned briefly the history and theory of precipitating serums and explained their use in identifying suspected animal proteins and in showing the blood relations of various animals. In discussing this communication A. H. JENNINGS explained how he had made use of the precipitin reaction in determining the kinds of animals bitten by biting flies. Dr. GEORGE W. FIELD and H. F. TAYLOR also took part in the discussion.

WILLIAM PALMER: *Porpoises and steamers.* Mr. Palmer commented on the frequency with which porpoises are found about the bows of steamers and advanced explanations as to their presence there and as to their methods of progression. His communication was illustrated by diagrams and lantern views of porpoises and other cetaceans. It was discussed by Dr. H. E. AMES.

M. W. LYON, JR., *Recording Secretary.*